animals testify to some corresponding mental states. If he will kindly refer to my original paper he will find that my views have not undergone the change he implies, for I *then* wrote: "We have therefore grounds for believing that, running parallel to the neuroses of animals, there are certain psychoses"; and again: "Animal minds are also ejective; they are more or less distorted images of our own minds"; and, in my "Conclusion," "While fully admitting the great interest that attaches to the study of the inferred mental faculties of the higher brutes," &c.

Were I to take his concluding remark seriously, and say that, if I were the only individual to hold the view that the mental life of animals cannot be the subject-matter of a science, this would not prove my view untrue, Mr. Romanes would smile at my want of appreciation of his powers of sarcasm. I content myself with drawing Mr. Romanes' attention, and that of your readers, to the following quotations from Prof. Huxley's volume on the Crayfish:—"Under these circumstances it is really quite an open question whether a crayfish has a mind or not; moreover, the problem is *an absolutely insoluble one*, inasmuch as nothing short of being a crayfish would give us positive assurance that such an animal possesses consciousness. . . . So we may as well leave this question of the crayfish's mind on one side for the present, and turn to *a more profitable investigation*," &c. (p. 89). And again: "At the most, one may be justified in supposing the existence of something approaching dull feeling in ourselves, and so far as such obscure consciousness accompanies the molecular changes of its nervous substance, it will be right to speak of the mind of a crayfish" (p. 126).

The question now seems to turn on what we mean by a science. Animal minds, as ejects, are distorted images of our own minds. Can we frame a *science* which deals with these distorted ejects? Could we frame a *science* of astronomy if the only method of procedure were to observe the stars and planets in mirrors *of varying and unknown curvature?* If we can give an affirmative answer to the latter question, I am ready to admit that, in the same degree, we can give an affirmative answer to the former.

C. LLOYD MORGAN

Circular Rainbow seen from a Hill-top

READING Mr. Fleming's letter in your issue of January 31 (p. 310), I am moved to put on record an observation of my own involving shadows and rainbows upon a cloud. On August 19, 1878, I was encamped upon a plateau known as Table Cliff, in the southern part of Utah Territory. The plateau has its longer dimension north and south, and ends southward in an acute promontory, precipitous toward the south, west, and east. The altitude is about 10,000 feet. On that day the air was moist, and scattering clouds were to be seen both in the valley beneath and in the sky above. A strong wind blew from the west. On that side of the promontory the air was clear; but at the crest a cloud was formed, so that the view eastward was completely cut off. This phenomenon is not unusual on mountain summits, and has been plausibly explained as due to the sudden rarefaction of the air on the lee-side of an obstacle. Standing on the verge of the cliff just before sunset, I saw my own shadow and that of the cliff distinctly outlined on the cloud. The figure appeared to be about fifty feet distant, and was not colossal. About the head was a bright halo with a diameter several times greater than the head. Its colours included only a portion of the rainbow series, but I neglected to record them, and do not venture to recite from memory. At the usual angle outside there appeared two rainbows of great brilliancy, likewise concentric with the head. They did not describe complete circles, but terminated at the left and beneath, where they met the shadow of the cliff. I estimated that 225° of arc were displayed. The phenomenon was continuous for some hours, the cloud-mass being persistent in position, notwithstanding the fact that its particles had a velocity of twenty-five or thirty miles an hour.

The observation has more than a scientific interest, because, in the popular imagination, the heads of scientific observers are not usually adorned with halos.

G. K. GILBERT

Washington, U.S.A., February 25

Right-sidedness

IN all the letters thus far published in NATURE on the subject of the tendency to deflection in walking, I find two things confounded which are quite distinct. There are two distinct senses in which we may use the term *right-leggedness*: the one refers to *strength*, the other to *dexterity* or accurate co-ordination of muscular action. In the arm these two always go together; for dexterity gives greater use (dexterity, I believe, is largely inherited), and use gives greater strength. But in the leg these may be and often are dissociated. As Prof. Darwin truly says, the left leg is often the stronger, but I believe the right is nearly always the more dexterous. My own case is a typical one. I hop on my left leg, and rise from it in jumping. But I do so not only because the left is stronger, but also, and I think mainly, because I use the right more dexterously as a swinging weight. The dexterous management of the free leg is certainly no less important than the strength of the jumping leg. In kicking or performing any other movement requiring dexterity, I stand on the left leg and use the right.

In my own case the whole body is *right-sided*, as far as dexterity is concerned. Impressions on my left eye are as vivid, perhaps even more vivid, than on my right, yet I see more intelligently (as, for example, in using a microscope) with my right. In the case of double images of near objects when looking at a more distant one, it is the left-eye image (the right in position) which I neglect. In pointing with the finger, whether of the right or left hand, with both eyes open, it is the right-eye image of the finger (the left in position) that I range with the object. In the case of two or three left-handed persons on whom I have made observations, I have found, on the contrary, that it is the right-eye image that they neglect, and the left-eye image that they use in pointing.

JOSEPH LE CONTE

Berkeley, California, February 19

"Suicide" of Black Snakes

WHILE encamped near Mount Wynne, Kimberley district, for a few days from June 13, 1883, our survey party saw and killed several black snakes averaging about five feet in length. In three days I saw seven of these unpleasant visitors in our camp. As is well known, the black snake is one of the most venomous of the Australian serpents, and whenever met with is if possible destroyed. I have seen many killed, but usually they die hard; and even when the back is broken in several places will linger for more than an hour, still capable of revenging themselves on an incautious assailant.

On this occasion our men had disabled one, and as I was anxious to obtain the skin I induced them to let it alone (they usually cut off the head so as to insure death). While we were looking at it some large black ants attacked the wounded part—about three feet from its head—when it instantly turned short round and hit itself twice in the neck, with seeming determination. In less than one minute it was dead. There can be no doubt, therefore, that it was poisoned by its own venom.

I do not know if such a custom on the part of snakes has been recorded. However, my men assured me that they had often witnessed similar occurrences, especially in the case of the "death" or "deaf" adder, a very venomous Australian snake. One man informed me that he had often insured the death of this reptile by simply pinning him to the ground by means of a forked stick. In all cases the reptile would turn round, bite himself, and die instantly.

EDWARD F. HARDMAN,
Government Geologist

Perth, Western Australia, January 28

Sea Fish in Freshwater Rivers

DURING my journey up the Fitzroy River with the surveying party from King's Sound to the Leopold Ranges (between lat. 17° 4′ and 18° 20′ S.), I observed many specimens of sword- and saw-fish. They appeared at intervals the whole way up the river, but none observed were more than three feet or three feet six inches long. About 300 miles up on the Margaret River I procured the saw of a small one. It measures about nine inches long and two inches wide. A few days after this, a little higher up the river, some of our men found a shark five feet long, and recently killed, probably by natives. I could not visit the place, as we were then about to break up camp for our return, but the men showed us some of the teeth, which were unmistakably those of a shark. They were, besides, well acquainted with the appearance of that fish.

Some time after this, when returning down a branch of the Fitzroy, and camped in the sand of the river bed, I found the

body of a small young shark. It was about eighteen inches long. I secured this as evidence. This locality is about 170 miles from the mouth of the river.

During the six months we were in the country, the bed of the river, which varies from 50 to 800 yards in width, was almost dry, with the exception of deep pools at intervals connected with each other by a narrow stream, often very shallow, running under the high banks. In the summer time the river is deeply flooded, the water rising ten to twenty feet (as shown by drift wood in trees) above the banks, in many places from forty to fifty feet high. The force of the flood might at its height prevent fish going up, but they could easily ascend in the intermediate season. In some cases the fish must have lived months in the upper waters, for portions of the Margaret, at least, are absolutely dry in the winter season, May to November usually.

I am not aware that such a circumstance has ever been noted before. If not, the fact is sufficiently interesting in itself. It is also important from a geological point of view, as showing that some caution must be observed in the classification of strata as freshwater or marine on the evidence of fish alone. No doubt many of these remains are embedded in the river detritus, and if discovered at some future time when the physical geology of the country has altered, might lead to the conclusion that these deposits were of marine origin.

EDWARD F. HARDMAN,
H.M. Geological Survey, Government Geologist
Perth, Western Australia, January 28

The Zodiacal Light

ONE of the members of the staff of this establishment, Mr. E. G. Constable, observed a brilliant appearance of the zodiacal light at about 7 p.m. on the evening of Wednesday the 5th inst., the cone of light being exceedingly well defined. The phenomenon was not visible long, having completely disappeared by 7.20 p.m. G. M. WHIPPLE
Kew Observatory, Richmond, Surrey, March 7

THE AXIOMS OF GEOMETRY

SINCE the time when Riemann and Helmholtz began their investigations on the axioms of geometry so much has been written on this subject in learned papers and in a more or less popular form that it might have appeared superfluous again to call the attention of writers on, and teachers of, elementary geometry to it, had it not been for the publication a year or two ago of a new edition of the first six books of Euclid's "Elements," with annotations and notes, by Prof. Casey. I hope the eminent author of this in many respects excellent book will excuse me for criticising some points in it, and making them the opportunity for again returning to the question about the axioms in geometry.

The points I object to besides his treatment of Book V., of which I may possibly say a few words on another occasion, is contained in Note B at the end of the book. Here Prof. Casey gives Legendre's and Hamilton's proofs of I. 32, that the sum of the interior angles of any triangle is equal to two right angles, implying, of course, that he considers these proofs valid, proofs which are independent of the theory of parallels. The theorem in question depends in Euclid upon Axiom XII., and all depends upon the question whether this axiom is necessary. For the two propositions in this axiom and in Theorem I. 32 stand in such a relation that either is a consequence of the other. Hence if I. 32 can be proved independently, the Axiom XII. changes into a theorem. But the investigations above referred to show that it is this axiom which tells us what kind of a surface the plane really is, and that until this axiom is introduced all propositions apply equally well to the spherical and to the plane surface.

I select for discussion the "quaternion proof" given by Sir William Hamilton, this being the easiest of the two. But that by Legendre can be treated in exactly the same way.

Hamilton's proof consists in the following :—

One side A B of the triangle A B C is turned about the point B till it lies in the continuation of B C; next, the line B C is made to slide along B C till B comes to C, and is then turned about C till it comes to lie in the continuation of A C. It is now again made to slide along C A till the point B comes to A, and is turned about A till it lies in the line A B. Hence it follows, *since rotation is independent of translation,* that the line has performed a whole revolution, that is, it has been turned through four right angles. But it has also described in succession the three exterior angles of the triangle, hence these are together equal to four right angles, and from this follows at once that the interior angles are equal to two right angles.

To show how erroneous this reasoning is—in spite of Sir William Hamilton and in spite of quaternions—I need only point out that it holds exactly in the same manner for a triangle on the surface of the sphere, from which it would follow that the sum of the angles in a spherical triangle equals two right angles, whilst this sum is known to be always greater than two right angles. The proof depends only on the fact, that any line can be made to coincide with any other line, that two lines do so coincide when they have two points in common, and further, that a line may be turned about any point in it without leaving the surface. But if instead of the plane we take a spherical surface, and instead of a line a great circle on the sphere, all these conditions are again satisfied.

The reasoning employed must therefore be fallacious, and the error lies in the words printed in italics; for these words contain an assumption which has not been proved. In fact they contain an axiom which completely replaces Euclid's Axiom XII., viz. it expresses that property of a plane which differentiates it from the sphere.

On the sphere it is, of course, not true that rotation is independent of translation, simply because every translation—sliding along a great circle—is a rotation about the poles of the great circle.

From this it might be said to follow that the calculus of quaternions must be wrong. But this again is not correct. The fact is that the celebrated author of this calculus had built it up with the full knowledge of the fundamental space properties in his mind, and making full use of them. Afterwards, on reasoning backwards, he got these space properties out of his formulæ, forgetting that they were exactly the facts with which he started. The process is, as far as logic is concerned, not very different from that practised by some alchemists, who pretended to make gold, and actually did produce gold out of their crucibles, but only as much as they had themselves put in.

The following considerations may help to clear up this point still further :—

Prof. Sylvester once conceived, in illustration of some points connected with our subject, an infinitely thin bookworm living in a surface, and consequently limited in its space conceptions to the geometry on such surface. In a similar manner we may imagine an intelligent being consisting merely of an eye occupying a fixed point in space, but capable of perceiving rays of light in every direction. For such a being space would have two dimensions only, but in this space it could conceive figures for which most of Euclid's definitions and all axioms with the exception of the twelfth, and therefore all propositions up to the twenty-sixth in the first book, would hold. Only the names *point*, *line*, *angle*, &c., would stand for objects different to those which they represent to our mind. Nothing can put the vagueness of Euclid's definitions and the real nature of his axioms, viz. that they contain the real logical definitions of the geometrical entities, in a clearer light than the fact that it is possible to use these so-called definitions for objects quite different from those to which Euclid applied them.

To return to our imaginary being: let us suppose it capable of studying Euclid. A ray of light, that is, a line,